some indication of their presence. This year I ascertained the positions of several of these showers with great care. The number of meteors from them averaged from 3 to 5 only, but the paths intersect nearly at a point in the individual cases, so that the centres are entitled to the same value as positions resulting from a large number of tracks. I give the best of these co-Lyrid showers, and the nearest confirmations from previous observations:—

Observed 1885	Previous Observations	Authority
April 18 ... 181° + 35½°	... 184° + 35°	... March 31-April 12, D.S., 1872.
19 ... 236 + 62	... 240 + 55	... April 14, Schiaparelli and Zezioli.
20 ... 226 + 41	... 223 + 40	... March 12-April 30, Greg and Herschel.
18-20 ... 296 ± 0	... 294 ± 0	... April 16-19, 1877, D.
19-20 ... 230 + 17	... 230 + 26	... April 20-24, Heis.
18-20 ... 299 + 24	... 298 + 25	... April 16-19, 1877, D.
	300 + 20	... April-May, Corder.
18-20 ... 213 + 9	... 206 + 13	... April 13-May 11, Heis.
18-20 ... 226 ± 0	... 228 − 2	... April 16-19, 1877, D.

The two radiants observed here in 1877 and 1885, with mean position at 295° ± 0° and 298°·5 + 24°·5, are very interesting. The former, just preceding η Aquilæ on the equator, supplies meteors of very great velocity, the latter in Vulpecula gives swift, streak-leaving meteors. This pair of showers, directed from points near the apex of the earth's way, are now, I believe, very exactly determined in regard to their centres of radiation. That they have hitherto evaded frequent detection is not surprising, as they only become well visible in the morning hours. It will be useful to watch for these special streams during future exhibitions of the Lyrids, as well as to note the several other interesting features closely associated with this well-known display.

W. F. DENNING

Chinese Insect Wax

THE beetle alluded to in connection with this subject in the last number of NATURE (vol. xxxi. p. 615) is a probably undescribed species of *Brachytarsus*, a genus of the family *Anthribidæ*, allied to the *Curculionidæ*. Through the courtesy of the authorities at Kew I have had specimens before me. The idea that it acts as a sort of midwife to assist at the birth of infant *Coccidæ* is quite erroneous. The genus *Brachytarsus* is a true parasite on *Coccus*, and its habits, in this connection, in Europe, have long been known. It is of course interesting to find "unity of habit" prevailing in the case of *Coccus Pé-la*, even to its parasite; but with regard to the latter there is nothing new; some points in the general economy of the wax insect, in the notes published, are of far greater importance.

Lewisham, May 1 R. MCLACHLAN

The New Bird in Natal

THERE can be little doubt from the description given by Mr. Turnbull in your issue of April 16 (p. 554) that the bird lately obtained by him in Natal is the Standard-winged Nightjar, *Cosmetornis* (seu *Macrodipteryx*) *vexillarius*, Gould. It has not been met with in Cape Colony, which accounts for Mr. Turnbull's inability to find mention of it in Layard's "Birds of South Africa;" but in Mr. Sharpe's new edition of Layard's work (which Mr. Turnbull would do well to procure) he will find an account of this bird given at p. 89. It appears to have a wide geographical range, being found both on the west and east coasts of Africa; in Angola and Damaraland, in Natal, on the Zambesi (where 300 miles up the river Dr. Kirk found it quite common), in the islands of Bourbon and Madagascar, along the Red Sea shore, and on the island of Socotra. With this extended range it is somewhat remarkable that it has not yet been met with in Cape Colony. According to the observations of Dr. Kirk the singular prolongations of the primaries are peculiar to the males, and a seasonal peculiarity observed only during the months from October until January. The habits of this bird, like those of other nightjars, are crepuscular. An excellent coloured figure of the male is given in Gould's "Icones Avium."

J. E. HARTING

Wild Bees

A FEW words respecting a colony of wild bees (a species of *Andrena*) which I have just discovered in our garden, may interest your entomological readers. A day or two ago, on walking beside a low-turfed mound which supports two trees on one of our towns, I noticed that the grassy surface on the south—therefore the sunny—side was covered with little hillocks of earth, such as ants throw up after rain. On examination each little heap showed the circular hole which denotes a bee's nest, and the bees themselves were seen in many places going in and out. Some holes were level with the ground, but most had the tiny mound of soil cast up in the process of excavation. The peculiarity of the case seems to me to lie in the great number of nests forming a complete colony. It is difficult to count them, but there cannot be less than eighty or ninety in an area—roughly calculated—of about sixty square feet. Have any of your readers noticed a similar city of these busy people? and can any one supply the specific name?

E. BROWN

Further Barton, Cirencester, May 2

ON M. WOLF'S MODIFICATION OF FOUCAULT'S APPARATUS FOR THE MEASUREMENT OF THE VELOCITY OF LIGHT

NO one who has the true interests of scientific accuracy at heart can fail to welcome any innovation whereby the elements of a research may be varied, for thereby the ever-lurking constant error is most readily eliminated. It seems, therefore, that this in itself is sufficient reason for the interesting paper communicated by M. Wolf to the Académie des Sciences (*Comptes Rendus*, 9 Février), describing a very ingenious arrangement of Foucault's experiment, and that there was no occasion for disparaging other work in order to justify its publication. It is to be hoped that this was done rather through inadvertence than design, but I feel called upon to correct some of the misapprehensions under which the author labours, and particularly those concerning the appearance and distinctions of the image of the slit in my work on the velocity of light.

M. Wolf remarks that, under the conditions which I selected, this image, even under the most favourable circumstances, must be bordered with very large diffraction fringes, which the atmospheric disturbances transform into a badly-defined "*tache lumineuse*." In reply to this, though I grant that the fringes *ought* to be present, yet I can affirm as a matter of fact that they were not to be seen. Possibly M. Wolf and others may have been somewhat misled by a drawing of the appearance of the image given in my work (p. 124, *Astr. Papers, American Ephemeris*, and *Nautical Almanac*, vol. i. Part 3) where the image proper, which is quite clear, is surrounded by a luminous haze, from which, however, it is very easily distinguished.

I hardly think that if M. Wolf had given the "specimen observations" (p. 133 of my work) due consideration, he would have characterised as a "*tache lumineuse*" an image whose position was measured with the following results (each result is the mean of ten observations made by one observer, and recorded without divulging the result by another):—

No. 1 ...	112·801 mm. ...	0·020 mm.
,, 2 ...	112·773 ,, ...	0·006 ,,
,, 3 ...	112·769 ,, ...	0·010 ,,
,, 4 ...	112·772 ,, ...	0·007 ,,
,, 5 ...	112·779 ,, ...	0·000 ,,
	Average difference from mean =	0·0086 ,,

These are measurements of the *deflected* image, so that the differences are not merely errors of linear measurement, but include errors in the estimate of the speed of the revolving mirror.

Now, M. Wolf, in his most sanguine statement, does not hope for a greater degree of accuracy than one part in 3500 in this particular measurement, whereas the above results are on the average closer than one part in 10,000.

But let us examine the data on which he bases this

most favourable estimate. In the first place, the image whose position is to be measured to within one-hundredth of a millimetre is the result of seventy-nine reflections from concave mirrors!

Secondly, one of these mirrors is to be 2 decimetres in diameter. Such a mirror used in a reflecting telescope would show signs of distortion if not carefully mounted—even at rest. But this mirror is required to make fifty revolutions per second, and the distortion is multiplied by forty reflections from its surface!

Finally, notwithstanding the avowed purpose of diminishing the path of the light ("*sans augmenter le trajet de la lumière*"), the distance required is *greater* than in my own experiments in the proportion of 1600 to 1200, and hence atmospheric disturbances would come into play in the same proportion—unless especial precautions were taken to guard against them.

And here, I am free to concede, is an important advantage, but one which is by no means limited to M. Wolf's arrangement, but is universally applicable—for by repeated reflection by plane or by concave reflectors the whole path, either in Fizeau's method or in Foucault's, may be confined to a limited space. But I think the chief object of such an arrangement—namely, to control easily the homogeneity of the air-column—could be more advantageously effected by a long underground tunnel containing a pipe, surrounded, if necessary, by running water, or, better still, exhausted of air.

At Prof. Newcomb's request I have repeated, with some alterations, the experiments described in the paper referred to, and occasionally the appearance of the image was better than in that work. On one occasion the width of the image was carefully measured, and found to be 0·25 mm. Evidently there is nothing remarkable in measuring the position of the centre of an image of this width within a hundredth of a millimetre.

Again, the "probable error" of my final result, 5 kilometres, would seem to show a somewhat greater degree of consistency than would be possible had I only a "*tache lumineuse*" to bisect.

I cannot forbear remarking that by astronomical methods—if M. Wolf entirely mistrusts the results obtained by Cornu, Newcomb, and myself—the velocity of light is known certainly within 1 per cent., and that it would, therefore, denote rather an excess of caution to deduce a formula for the elimination of a possible uncertainty of from 5 to 10 per cent., as M. Wolf does in determining "*l'ordre* M *de cette déviation.*"

In conclusion, I think M. Wolf is to be congratulated on the very happy combination he has devised for the solution of this most fascinating problem—a problem which, notwithstanding its difficulties, will ultimately yield a result correct not merely to one part in 3500, but, I firmly believe, one in 300,000—perhaps one in 1,000,000.

ALBERT A. MICHELSON

SELF-INDUCTION IN RELATION TO CERTAIN EXPERIMENTS OF MR. WILLOUGHBY SMITH, AND TO THE DETERMINATION OF THE OHM

IN a lecture delivered by Mr. Willoughby Smith before the Royal Institution in June last (see *Proceedings*) some experiments are detailed, which are considered to afford an explanation of discrepancies in the results of various investigators relating to the ohm, or absolute unit of electrical resistance. As having given more attention than probably any one else in recent years to this subject, I should like to make a few remarks upon Mr. Willoughby Smith's views, which naturally carry weight corresponding to the good service done by the author in this branch of science.

In the first series of experiments a primary circuit is arranged in connection with a battery and interrupter, and a secondary circuit in connection with a galvanometer and commutator of such a character that the make and break induced currents pass in the same direction through the instrument. Under these circumstances it is found that at high speeds the insertion of a copper plate between the primary and secondary spirals entails a notable diminution in the galvanometer deflection, and this result is regarded as an indication that the molecules of copper need to be polarised by the lines of force—an operation for which there is not time at the higher speeds. The orthodox explanation of the experiment would be that currents are developed by induction in the copper sheet, which thus screens the secondary spiral from the action of the primary, and the result is exactly what might have been anticipated from known electrical principles. I have the less hesitation in saying this, because as a matter of fact I did anticipate from theory the action of a combination very similar in character. The experiment is described in the *Philosophical Magazine* for May, 1882, and differs from Mr. W. Smith's only in the substitution of a telephone for the galvanometer, and of a microphone for the interruptor, no reverser in the secondary circuit being required. By the interposition of a thick copper sheet the sound is greatly enfeebled.

The second series of experiments were made with Faraday's "new magneto-electric machine," in which a copper disk rotates about its centre between the poles of a horse-shoe magnet. The currents developed are examined with a galvanometer whose electrodes touch two points upon the disk—in Mr. W. Smith's experiments, one at the centre, and the other at the circumference. At low speeds the distribution is symmetrical with respect to that diameter of the disk which is passing at any moment between the poles; but, as the speed is increased, a certain "drag" is observed, disturbing the symmetry. This drag, or lagging, was noticed by Nobili in a very similar arrangement as long ago as 1833 ("Wiedemann's Electricity," third edition, vol. iv., § 374), and is no doubt to be attributed to the induction of the currents upon themselves.

This question of self-induction is indeed a very important one in respect of certain methods for determining the ohm; but it certainly cannot be said to have been neglected, as Mr. W. Smith seems to suggest. Both in the original experiments of the British Association Committee with a coil revolving about a vertical axis, and in my own recent repetition of them, the self-induction of the coil is a most important feature, and may cause a displacement of the position of maximum current from the plane of the magnetic meridian through as much as 20°. In my paper (*Phil. Trans.*, 1882, p. 661) I thought I had discussed the question at almost tedious length.

It is possible that Mr. W. Smith had in his mind rather determinations by the method of Lorenz, in which Faraday's disk is used. The arrangement here, however, differs in one very important respect from that of Mr. W. Smith's experiments in that the lines of force are symmetrically arranged in relation to the axis of rotation. The consequence is that, however great the speed of rotation, there are no currents circulating in the disk, and therefore no question arises as to the self-induction of such currents. What is observed is simply the difference of electrical potential between the centre and the circumference. It is impossible to discuss the matter fully here, but the reader will find all that is necessary by way of explanation in the paper published in the *Phil. Trans.* ("Experiments by the Method of Lorenz for the further Determination of the Absolute Value of the British Association Unit of Resistance," &c.). My object in writing is to correct the inference, suggested by W. Smith's remarks, that the question of self-induction has been neglected by workers upon this subject.

RAYLEIGH